PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Violence against Congolese Refugee Women in Rwanda and Mental Health: A Cross-Sectional Study using Latent Class Analysis
AUTHORS	Sipsma, Heather; Falb, Kathryn; Willie, Tiara; Bradley, Elizabeth; Bienkowski, Lauren; Meerdink, Ned; Gupta, Jhumka

VERSION 1 - REVIEW

REVIEWER	Susan Bartels Queen's University, Kingston, Canada Harvard Humanitarian Initiative, Boston, USA FXB Center for Health and Human Rights, Boston, USA
REVIEW RETURNED	24-Sep-2014

GENERAL COMMENTS	Recommend revising the references to include more DRC relevant data. I disagree that the emotional distress described in this study is higher than previously reported as the authors discuss in their manuscript. I also do not entirely agree with the conclusion in its current form. From my perspective, the timing of the IPV and whether it is ongoing is important and this is not considered as a variable in the study. Introduction: In the second sentence of the Introduction you mention the magnitude of conflict related violence and cite references from East Timor, Sierra Leone, Azerbaijan, and Cote d'Ivoire. Despite the fact that the current study is on Congolese women and that there is a great deal of literature documenting violence in DRC, I wonder why you would omit more relevant DRC studies? (Johnson, et al JAMA 2010 and Peterman et al AM J Public Health 2011 are both important studies but there are others) Are there any relevant studies on mental health outcomes among Congolese refugees or on Congolese women who are displaced within DRC (i.e. IDPs)? It does exist for Congo in general (not necessarily displaced), for example in the study by Johnson, et al. This data may not be available for displaced Congolese populations but you could add the Congolese data that does exist and / or comment on the lack of data for displaced Congolese population. Methods: It was not clear to me exactly when the data was collected (although RHA was rolled out sometime in 2008)? Rather than writing "At the time of the survey….", suggest saying, "In October – November 2008…." Or whatever the case might be. You outline the sociodemographic controls included but it seems like a socioeconomic indicator / proxy might also be important

	(household income, number meals / day, material that home is constructed with). Results / Discussion: IPV may have had a more notable impact because it is still ongoing and the timing of the IPV in relation to emotional distress is the one important consideration that I think is missing in this study. This impacts the conclusion I think. If conflict related violence were still ongoing then perhaps its emotional effects would be equal to or greater than those of IPV? We cannot tell from this work because the conflict related violence was in the distant past for most (presumably, since they fled the conflict in DRC). Recall bias is mentioned for IPV since the study considers lifetime IPV but it is also a consideration for conflict related violence since that extends back quite a ways, perhaps to 1996. Less severe forms of violence may not be reported as a result. Your finding that 17.5% of participants report sexual violence seems low in comparison to some other estimates from DRC. Do you think this is because of under-reporting (stigma, etc)? Do you think that these women were less likely to experience sexual violence because they fled the conflict, seeking asylum in Rwanda? Please comment in the manuscript on your findings vs. higher numbers reported elsewhere for DRC. You report that 1 in 4 women have poor mental health outcomes and conclude that this is higher than documented elsewhere, citing studies from Cote d'Ivoire, Thai Burma, and Liberia. Again, I think it's important to look at data pertaining to the population that you are studying and I would disagree with your conclusion that 1 in 4 is higher than previously reported in DRC. In the JAMA study (Johnson et al), 67% screened positive for depression, 76% screened positive for PTSD, 37% screened positive for suicidal ideation and 33% screened positive for suicide attempt. Realizing that your measure of mental health was different, I think the conclusion is still inaccurate and would recommend revising this aspect of the discussion. I agree that conflict and humanitarian programming and funding should not focus on conflict related violence to the exclusion of IPV. My concern is that the timing of the violence plays an important role in determining emotional distress and that IPV is more likely to be ongoing, so the comparison is a little unfair. However, in arguing for IPV programming in addition to programming for conflict related violence, I would bolster the argument by again re-iterating that prevalences of IPV are often higher in complex emergencies and that many women report IPV after experiencing conflict related violence. Also the IPV is more likely to be ongoing violence and may therefore be more amenable to intervention / prevention.

REVIEWER	Dr. Basia Tomczyk US Centers for Disease Control and Prevention USA
REVIEW RETURNED	24-Nov-2014

GENERAL COMMENTS	It is stated: "ARC program staff used household lists within the pre-defined catchment area for the sampling frame" Please indicate how lists were compiled and when the lists were last updated.

	Please describe how you calculated your sample size and include the sampling parameters that you used. It is stated: "Excel and/or a random numbers table were used to randomly select eligible households and individuals". Did you do a systematic random sample selection or a simple random sample selection? Please indicate if you sampled based on probability proportional to size of the camps. It is stated: "randomly selected one woman of reproductive age (15-49 years of age) within the household". Please describe the method you used to randomly select WRA. Please describe in your methods how you handled WRA that may have needed counseling. Please indicate if you followed the WHO guidelines on conducting violence surveys. How many trained female interviewers were used? What type of experience did they have? How did you train them for this type of survey? It is stated: "Participants were asked to report whether or not they experienced these types of violence 1) during the conflict and 2) after the conflict (with specific dates to indicate violence experienced after fleeing the conflict)". Please indicate the specific dates you used for 1 and 2. It is stated" The secondary data analysis of unidentifiable data was deemed exempt from review by the Yale School of Public Health human subjects committee". This is not clear. Was another study protocol approved by an IRB? Is this only secondary data analysis of a larger study? Did the larger study have IRB approval? By whom? Results: Please include how many women of those selected completed the study, how many refused, etc. Please indicate your final sample size. Limitations should address interviewer bias, recall bias, sampling bias ie when was list last updated. Is it possible the women interviewed misinterpreted questions?

VERSION 1 – AUTHOR RESPONSE

Comments from Reviewer 1

Comment 1
Recommend revising the references to include more DRC relevant data.

Response
We appreciate the suggestion and have included the following additional references for integrating more DRC relevant data.

Hanlon H. Implications for health care practice and improved policies for victims of sexual violence in the Democratic Republic of Congo. Journal of International Women's Studies. 2008;10(2):64-72.

Human Rights Watch. Soldiers Who Rape, Commanders Who Condone: Sexual Violence and Military Reform in the Democratic Republic of Congo. New York, NY; 2009.

Kelly, J. T., Betancourt, T. S., Mukwege, D., Lipton, R., & VanRooyen, M. J. Experiences of female survivors of sexual violence in eastern Democratic Republic of Congo: A mixed-methods study. Conflict and Health. 2011;5(25):2-8.

Ministry of Planning, Democratic Republic of Congo. Demographic and Health Survey 2007. Calverton, MD; 2008.

Pham P, Vinck P, Weinstein H. Human rights, transitional justice, public health and social reconstruction. Social Science & Medicine. 2010;70(1):98-105.

Pratt, M. & Werchick, L. Sexual terrorism: Rape as a weapon of war in Eastern Democratic Republic of Congo. USAID/DCHA Assessment Report; 2004.

Tlapek SM. Women's status and intimate partner violence in the Democratic Republic of Congo. Journal of Interpersonal Violence. 2014;epub before print. 1-15.

Comment 2
I disagree that the emotional distress described in this study is higher than previously reported as the authors discuss in their manuscript. I also do not entirely agree with the conclusion in its current form. From my perspective, the timing of the IPV and whether it is ongoing is important and this is not considered as a variable in the study.

Response
We appreciate the reviewer's comments. We aim to make comparisons with Congolese refugees in other settings. We have found a recently published article on the mental health of Congolese refugees living in Uganda, which, as the reviewer suspected, is higher than our estimate of emotional distress. We have included this comment in our Discussion on Page 10 of the tracked manuscript:

This prevalence, however, is lower than estimate of depression (54%) found among refugees living in Uganda, who were primarily from the Democratic Republic of Congo (26).

Morof D, Sami S, Mangeni M, Blanton C, Cardozo B, Tomczyk B. A cross-sectional survey on gender-based violence and mental health among female urban refugees and asylum seekers in Kampala, Uganda. International Journal of Gynecology and Obstetrics. 2014;127:138-143.

We have also included the inability to know whether or not the IPV is ongoing as a limitation on Page 12:

Additionally, the experience of IPV may be ongoing... this analysis lacks the ability to make conclusions about the effects of chronic IPV. Additional research is needed to overcome these limitations in order to better understand the timing of such events.

Comment 3
Introduction: In the second sentence of the Introduction you mention the magnitude of conflict related violence and cite references from East Timor, Sierra Leone, Azerbaijan, and Cote d'Ivoire. Despite the fact that the current study is on Congolese women and that there is a great deal of literature

documenting violence in DRC, I wonder why you would omit more relevant DRC studies? (Johnson, et al JAMA 2010 and Peterman et al AM J Public Health 2011 are both important studies but there are others)

Response
The reviewer makes an important point. We have added both Johnson et al. (2010) and Peterman, Palermo, and Bredenkamp (2011) to this list of citations and have also included Tlapek (2014); Pratt & Werchick (2004); Pham, Vinck, and Weinstein (2010); and Kelly et al. (2011).

Comment 4
Are there any relevant studies on mental health outcomes among Congolese refugees or on Congolese women who are displaced within DRC (i.e. IDPs)? It does exist for Congo in general (not necessarily displaced), for example in the study by Johnson, et al. This data may not be available for displaced Congolese populations but you could add the Congolese data that does exist and / or comment on the lack of data for displaced Congolese population.

Response
We appreciate this comment and have found a recently published article on the mental health of Congolese refugees living in Uganda. We have added this reference to our Discussion on Page 10 of the tracked manuscript:

This prevalence, however, is lower than estimate of depression (54%) found among refugees living in Uganda, who were primarily from the Democratic Republic of Congo (26).

Comment 5
Methods: It was not clear to me exactly when the data was collected (although RHA was rolled out sometime in 2008)? Rather than writing “At the time of the survey….”, suggest saying, “In October – November 2008….” Or whatever the case might be.

Response
We have made the change as recommended. The sentence on Page 5 of the tracked manuscript now reads as follows:

In July and August 2008, 27,088 refugees lived in these two camps.

Comment 6
You outline the sociodemographic controls included but it seems like a socioeconomic indicator / proxy might also be important (household income, number meals / day, material that home is constructed with).

Response
We agree and believe that socioeconomic status prior to displacement would be an important factor to consider. This information, however, was not collected in the survey. To ascertain SES at the time of data collection, we contend that the refugee camp would be the best indicator of socioeconomic status as each camp is fairly homogenous. We have added this point to the limitations in the Discussion on Page 12 of the tracked manuscript:

We also did not include socioeconomic status, as status prior to displacement was unavailable; socioeconomic status has been associated with mental health in previous research and is an important factor to consider for future work (28).

Comment 7

Results / Discussion: IPV may have had a more notable impact because it is still ongoing and the timing of the IPV in relation to emotional distress is the one important consideration that I think is missing in this study. This impacts the conclusion I think. If conflict related violence were still ongoing then perhaps its emotional effects would be equal to or greater than those of IPV? We cannot tell from this work because the conflict related violence was in the distant past for most (presumably, since they fled the conflict in DRC).

Response
The reviewer is correct that IPV may be ongoing, while the conflict violence is in the relatively distant past. We have added this point as an important consideration in the limitations section on Page 12:

Additionally, the experience of IPV may be ongoing, whereas violence experienced during the conflict is relatively distant and violence experienced after the conflict may be less distant but still in the past. These possible discrepancies in the timing of violence occurrence could help explain the impact of IPV on emotional distress within this sample. Additional research is needed to further understand the timing of such events.

Comment 8
Recall bias is mentioned for IPV since the study considers lifetime IPV but it is also a consideration for conflict related violence since that extends back quite a ways, perhaps to 1996. Less severe forms of violence may not be reported as a result.

Response
The reviewer brings up an important point. We have therefore added the following sentence as a limitation on Page 12 of the tracked manuscript:

Third, recall bias could have affected the reporting of both IPV as well as conflict-related violence, given that the conflict arose in the early 1990s, and could have caused underreporting of less severe forms of violence.

Comment 9
Your finding that 17.5% of participants report sexual violence seems low in comparison to some other estimates from DRC. Do you think this is because of under-reporting (stigma, etc)? Do you think that these women were less likely to experience sexual violence because they fled the conflict, seeking asylum in Rwanda? Please comment in the manuscript on your findings vs. higher numbers reported elsewhere for DRC.

Response
We agree that some estimates of sexual violence from the DRC are higher than our findings. Many of these estimates, however, are from clinic-based samples, which tend to have higher reports of some forms of sexual violence than more general samples. Furthermore, this estimate of 17.5% is the prevalence of sexual violence during the conflict; approximately 7% of participants reported sexual violence after the conflict and 14% reported sexual IPV. These estimates are generally comparable to population-based surveys when the various types of sexual violence are accounted for. We have added the following sentence to explain the comparability of our findings on Page 11 of the tracked manuscript:

Estimates of conflict-related sexual violence among this sample were comparable to other general population samples from the DRC (4, 8, 27).

Comment 10
You report that 1 in 4 women have poor mental health outcomes and conclude that this is higher than

documented elsewhere, citing studies from Cote d'Ivoire, Thai Burma, and Liberia. Again, I think it's important to look at data pertaining to the population that you are studying and I would disagree with your conclusion that 1 in 4 is higher than previously reported in DRC. In the JAMA study (Johnson et al), 67% screened positive for depression, 76% screened positive for PTSD, 37% screened positive for suicidal ideation and 33% screened positive for suicide attempt. Realizing that your measure of mental health was different, I think the conclusion is still inaccurate and would recommend revising this aspect of the discussion.

Response
We appreciate the reviewer's comment and, in light of additional literature on Congolese refugees, have revised this aspect of the Discussion. The beginning of the Discussion on Page 10 now reads as follows:

One in four women were thought to be experiencing poor mental health, an estimate higher than other reported estimates of poor mental health, including past-month suicidality (7%) (7), PTSD (12%-14%) (2, 11), and depression (11%) (11) among women in conflict-affected settings. This higher prevalence seems likely attributable to the more inclusive nature of our mental health measure. This prevalence, however, is lower than the estimate of depression (54%) found among refugees living in Uganda, who were primarily from the Democratic Republic of Congo (26). This discrepancy could be explained by differences in the respondents; the Congolese refugees living in Rwanda had been residing in the camps longer on average than the Congolese refugees in Uganda.

Comment 11
I agree that conflict and humanitarian programming and funding should not focus on conflict related violence to the exclusion of IPV. My concern is that the timing of the violence plays an important role in determining emotional distress and that IPV is more likely to be ongoing, so the comparison is a little unfair. However, in arguing for IPV programming in addition to programming for conflict related violence, I would bolster the argument by again re-iterating that prevalences of IPV are often higher in complex emergencies and that many women report IPV after experiencing conflict related violence. Also the IPV is more likely to be ongoing violence and may therefore be more amenable to intervention / prevention.

Response
The reviewer makes an important point. We have thus added the following text on Pages 13 and 14 of the tracked manuscript:

Programs aimed to respond to violence against women should thus be equipped to respond to the mental health needs of women with varying levels of exposure to both discrete events of violence that are more common with conflict-related violence as well as chronic, continual exposure to IPV as many women in conflict-affected settings experience ongoing IPV.30 Studies also suggest that the prevalence of IPV may be higher in conflict-affected settings and that exposure to conflict may increase a woman's vulnerability to experiencing IPV. As such, prevention and intervention programs are needed to reduce the prevalence of IPV and mitigate its health consequences. To date, little data exist regarding promising interventions in such conflict affected settings, but at least two randomized trials within Cote d'Ivoire demonstrate the potential to reduce IPV in conflict affected settings (2, 12). More research is needed in this area.

Comment from Reviewer 2

Comment 1
It is stated: "ARC program staff used household lists within the pre-defined catchment area for the

sampling frame"
Please indicate how lists were compiled and when the lists were last updated.

Response
We have included this information as requested in the manuscript on Page 5 of the tracked manuscript:

ARC program staff used household lists, compiled through home visits and updated monthly, within the pre-defined catchment area for the sampling frame…

Comment 2
Please describe how you calculated your sample size and include the sampling parameters that you used.

Response
We used all available respondents that fit our inclusion criteria and did not use sample size calculations for this analysis of secondary data. We have included this information on Page 6 of the tracked manuscript:

We used all available data and restricted our analytic sample to ever-married women (N=548)…

Comment 3
It is stated: "Excel and/or a random numbers table were used to randomly select eligible households and individuals". Did you do a systematic random sample selection or a simple random sample selection?

Response
The American Refugee Committee (ARC) used simple random sampling. We have clarified this in the following sentence found on Page 5 (underlined is new):

Excel and/or a random numbers table were used to randomly select eligible households and individuals using simple random sampling.

Comment 4
Please indicate if you sampled based on probability proportional to size of the camps.

Response
No, sampling was not based on probability proportional to size of the camps. We have added this point as a limitation on Page 13 of the tracked manuscript.

Last, sampling was not proportional to the size of the refugee camps…

Comment 5
It is stated: "randomly selected one woman of reproductive age (15-49 years of age) within the household". Please describe the method you used to randomly select WRA.

Response
We have briefly included information on this process and a reference for additional details on Pages 5 and 6 of the tracked manuscript.

The sampling strategy to select women of reproductive age was implemented per technical

assistance from the CDC and their guidelines in the Toolkit (21). First, the program staff asked the adult who answered the door how many women between the ages 15 and 49 lived in the household. If more than one eligible woman lived in the household, the interview staff listed all eligible women and their ages from oldest to youngest and looked up the random number in the selection table to indicate which woman to be interviewed. Additional details about this process and associated forms can be found elsewhere (21).

Comment 6
Please describe in your methods how you handled WRA that may have needed counseling.

Response
We have included this information in our Methods section on Page 6.

Participants who may have needed counseling were referred to the ARC office in each of the camps, where women were linked with appropriate resources.

Comment 7
Please indicate if you followed the WHO guidelines on conducting violence surveys.

Response
Yes, and we have made note of this in our manuscript on Page 6:

The ARC followed the World Health Organization's guidelines for conducting violence surveys (22).

Comment 8
How many trained female interviewers were used? What type of experience did they have? How did you train them for this type of survey?

Response
Female interviewers interviewed all female participants. Interviewers were selected from community volunteers, mainly community health workers and peer educators. They were trained using guidelines outlined in the CDC's Reproductive Health Assessment Toolkit for Conflict-Affected Women (Chapter 4). Unfortunately, we do not have the exact number of female interviews for the survey. The following text has been added to Page 5 of the tracked manuscript:

Trained female interviewers, selected mainly from community health workers and peer educators and trained using guidelines outlined in the CDC's Reproductive Health Assessment Toolkit for Conflict-Affected Women (Toolkit) (21), visited each selected household and randomly selected one woman of reproductive age (15-49 years of age) within the household.

Comment 9
It is stated: "Participants were asked to report whether or not they experienced these types of violence 1) during the conflict and 2) after the conflict (with specific dates to indicate violence experienced after fleeing the conflict)". Please indicate the specific dates you used for 1 and 2.

Response
We appreciate this comment and have inserted the dates into the text. The statement on Page 7 of the tracked manuscript now reads as follows:

Participants were asked to report whether or not they experienced these types of violence 1) during the conflict (October 1996 – March 2003) and 2) after the conflict (after March 2003).

Comment 10
It is stated" The secondary data analysis of unidentifiable data was deemed exempt from review by the Yale School of Public Health human subjects committee". This is not clear. Was another study protocol approved by an IRB? Is this only secondary data analysis of a larger study? Did the larger study have IRB approval? By whom?

Response
This manuscript reports results of a secondary data analysis from a larger data collection effort led by the American Refugee Committee (ARC). The purpose of ARC's data collection effort was to inform their internal programming. As such, ARC did not seek IRB approval. We have included the following text describing the IRB approval for the secondary analysis on Page 6:

This secondary data analysis of unidentifiable data was deemed exempt from review by the Yale School of Public Health human subjects committee and the Internal Review Board at the University of Illinois at Chicago.

Comment 11
Results: Please include how many women of those selected completed the study, how many refused, etc. Please indicate your final sample size.

Response
A total of 548 women were included in this analysis (Page 6). Unfortunately, we do not know the participation or refusal rate for this study. We have included this point as a limitation on Page 13.

(Page 6)
We used all available data and restricted our analytic sample to ever-married women (N=548)…

(Page 13)
Last…the survey's participation rate is unknown.

Comment 12
Limitations should address interviewer bias, recall bias, sampling bias ie when the list was last updated. Is it possible the women interviewed misinterpreted questions?

Response
We agree with the limitations suggested by the reviewer and appreciate this comment. We have added these points in our limitations section on Pages 12 and 13 of the tracked manuscript as shown below:

Despite several strengths of our study, including a large, probability-based sample of refugee women, findings should be interpreted in light of some limitations... Third, recall bias could have affected the reporting of both IPV as well as conflict-related violence, given that the conflict arose in the early 1990s, and could have caused underreporting of less severe forms of violence. Women may have also misinterpreted questions; however, survey items were based on the WHO's domestic violence and women's health surveys, which have been field tested in several vulnerable populations (22, 29). Further, women may have also underreported experiences of violence due to stigma associated with these experiences. All interviews, however, were conducted in private, safe spaces, and confidentiality was reinforced throughout the survey to encourage unbiased reporting. Interviewer bias is also a possibility; however, all interviewers were trained according to WHO guidelines for conducting domestic violence surveys (22, 29). All interviewers were able to offer participants ethical referrals for relevant services according to each participant's wishes, and the benefits of seeking assistance from qualified service providers were continuously reinforced throughout the refugee

camps… Last, sampling was not proportional to the size of the refugee camps and the survey's participation rate is unknown.

VERSION 2 – REVIEW

REVIEWER	Susan Bartels Faculty, Harvard Humanitarian Initiative Fellow and Visiting Scientist, FXB Center for Health and Human Rights Clinician Scientist, Queen's University
REVIEW RETURNED	03-Mar-2015

GENERAL COMMENTS	It was a pleasure to review your revised manuscript entitled, "Violence against Congolese Refugee Women in Rwanda and Mental Health: A Cross-Sectional Study using Latent Class Analysis". Earlier questions and comments were all sufficiently addressed and the manuscript has been revised to reflect the suggested changes. The introduction and discussion now include more relevant comparisons and citations. Furthermore, limitations such as inability to account for the potential ongoing nature of IPV have been brought forth. I am pleased to recommend approval of this manuscript.

REVIEWER	Basia Tomczyk US Centers for Disease Control and Prevention, USA
REVIEW RETURNED	23-Feb-2015

GENERAL COMMENTS	You may remove this sentence from the text: "First, the program staff asked the adult who answered the door how many women between the ages 15 and 49 lived in the household. If more than one eligible woman lived in the household, the interview staff listed all eligible women and their ages from oldest to youngest and looked up the random number in the selection table to indicate which woman to be interviewed". YOu can state that the KISH method was used in place of this sentence. Please indicate if the questionnaire was translated and back-translated. On Page 5 it is stated: "Emotional distress was measured using the Self Report Questionnaire-20 (SRQ-20) (23), which had been developed as a screening tool by the World Health Organization". Please consider adding " as a mental health screening tool"... On page 9 it is stated: "This higher prevalence seems likely attributable to the more inclusive nature of our mental health measure". Please describe what is meant by "more inclusive nature of our mental health measure? This is not clear. What are you comparing it to?

VERSION 2 – AUTHOR RESPONSE

Comments from Reviewer 1

Comment 1
You may remove this sentence from the text: "First, the program staff asked the adult who answered the door how many women between the ages 15 and 49 lived in the household. If more than one eligible woman lived in the household, the interview staff listed all eligible women and their ages from oldest to youngest and looked up the random number in the selection table to indicate which woman to be interviewed". You can state that the KISH method was used in place of this sentence.

Response
We have removed the sentence from the text as the reviewer has suggested. The text on Page 5 now reads as follows:

The sampling strategy to select women of reproductive age used the KISH method (22) and was implemented per technical assistance from the CDC and their guidelines in the Toolkit (21).

Comment 2
Please indicate if the questionnaire was translated and back-translated.

Response
Yes, the questionnaire was translated and back-translated. We have included this information in the text on Page 6:

Surveys were translated and back-translated to ensure accuracy and collected data on a variety of topics…

Comment 3
On Page 5 it is stated: "Emotional distress was measured using the Self Report Questionnaire-20 (SRQ-20) (23), which had been developed as a screening tool by the World Health Organization". Please consider adding " as a mental health screening tool"...

Response
We agree with the reviewer and have improved the clarity of this sentence on Page 6 as follows:

Emotional distress was measured using the Self Report Questionnaire-20 (SRQ-20) (23), which had been developed as a mental health screening tool by the World Health Organization.

Comment 4
On page 9 it is stated: "This higher prevalence seems likely attributable to the more inclusive nature of our mental health measure". Please describe what is meant by "more inclusive nature of our mental health measure? This is not clear. What are you comparing it to?

Response
We agree that this sentence requires additional clarity. We have revised the text on Page 10 to read as follows:

This higher prevalence seems likely attributable to the more inclusive nature of our mental health measure, which assesses general emotional distress as opposed to specific diagnoses.